"NUCLEAR WINTER": A BRIEF REVIEW AND COMMENTS ON SOME RECENT LITERATURE

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PREFACE

From the initial publication in 1983 of the paper by Turco, Toon, Ackerman, Pollock, and Sagan (generally known as TTAPS) that put forth the concept and the attention-getting name of "nuclear winter," the late Diran Deirmendjian had a deep interest in the topic. His previous research over a number of years into the problems of atmospheric radiation, especially scattering by aerosol particles, and into the meteorological consequences of volcanic dust spewed into the upper atmosphere by large eruptions, gave him considerable expertise. His work in RAND's Climate Dynamics Project in the early 1970s was also directly applicable to the nuclear winter studies. He, along with many other specialists in atmospheric radiation, felt that the model used by the TTAPS team was much too simplistic to justify the dire predictions they made. Even when later investigators developed more elaborate models, he held to his opinion that the actual processes of absorption, scattering, and reflection were far more complicated and the end result considerably less certain.

In particular, most of the models assumed that the absorption of radiation by the smoke layer would lead to the heating of the atmosphere at that level, and the ensuing buoyancy would cause the smoke to rise still more and lengthen its residence time. He pointed out that the smoke particles are so small by comparison to the wave length of light that the black body radiation law does not hold. Thus, he found no physical process to create the hypothesized heating.

Although he never had the support for a serious study into these problems, he kept up with the literature on the topic, which became quite voluminous, and so he resolved to write the present paper in an attempt to clarify some of the issues that became bogged down in controversy. At the time of his death the paper was all but complete, but it was set aside and overlooked for a number of months. When it came to my attention, I decided to try to complete the process of publication with the hope of getting on the record the thoughts of a

person who was singularly qualified to study the problem. Although Deirmendjian did not have the facilities or time to consider in detail all of the literature on the subject of nuclear winter, and although there has been some (but apparently not much) additional progress on the topic since his death, I think it is worthwhile for his paper to be published at this time.

F. W. Murray
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"NUCLEAR WINTER": A BRIEF REVIEW AND COMMENTS ON SOME RECENT LITERATURE

Introduction: This is a follow-up to a memo [Deirmendjian, 1984] on the subject of *nuclear winter* (NW), a term introduced by the so-called TTAPS authors in their elaboration [Turco, et al., 1983] of an original study by Crutzen and Birks [1982] on the climatic effects to be expected after a major nuclear conflict. A number of related new studies, reviews, comments, and discussions have since appeared in print. The present piece is a slightly expanded version of a brief review of--and commentary on--this literature originally incorporated in another internal RAND memo [Deirmendjian, 1985].

In what follows, I offer a brief review with comments on some of the more recent NW-related literature I was able to scan through my own initiative and unsponsored efforts to keep abreast of new developments on the subject. These notes do not by any means represent a complete and detailed analytical review of the current literature on NW work.

1. **Major reviews:** The most comprehensive and authoritative review and discussion of NW efforts available so far is probably that produced by a special committee appointed by the *National Research Council of the National Academy of Science* [NAS/NRC, 1985]. Despite its large membership (eighteen, which, by the way, included two of the original TTAPS authors), the committee's report is, by and large, remarkably clear and cohesive, at least in those parts I read in detail.

The committee's general conclusion suggests that some non-trivial atmospheric effect resulting from a major nuclear exchange must be expected, but due to the complicated nature of the corresponding mathematical problem, its exact nature, magnitude, extent, and duration are difficult to predict, even if we were to possess more precise information on certain key parameters.

After emphasizing the considerable and invalidating uncertainties involved, both in such parameters and in the mathematical modeling, the report arrives at certain other conclusions on the premise of "we don't know, but let's pretend we do and go ahead"--a practice which seems to be common in some of the atmospheric sciences [see e.g., NAS/NRC, 1985, p. 127].

A comprehensive summary of all the aspects of the problem discussed in the NAS report is not possible here. Instead let me touch lightly upon a few points that are related to my own field. An example is the use of the effects of major volcanic events as possible analogs for NW effects [NAS/NRC, 1985, pp. 17, 127, 175 ff].

The report [NAS/NRC, 1985, p. 17] cites figures of $0.3 \bullet 10^6$ tons of dust lofted per megaton (MT) of explosion and 330 to $825 \bullet 10^6$ tons of "dust" lofted by "1500 MT of surface bursts." This may be compared with my own earlier estimates [Deirmendjian, 1973a] of 30, 13, and 9 million tons of dust injected respectively by Krakatoa (1883), Katmai (1912), and Agung (1973) volcanoes. As may be seen, the mass ratio between Krakatoa's ejecta (largest known volcanic explosion, as described e.g., by Simkin & Fiske [1983]) and the NAS/NRC lower bound of $330 \bullet 10^6$ tons of material lofted by a 1500 MT nuclear exchange is just about 1 to 10; and if the corresponding optical thickness anomaly τ_D were simply mass-dependent (which is not quite true), on the basis of Krakatoa's estimated [Deirmendjian, 1973a] value of $\tau_D \cong 0.63$, the nuclear stuff would yield values in the range $7 \leq \tau_D \leq 17$, instead of the hemispherical value of 4 given by the NAS/NRC. Such values, by the way, can be found typically in ordinary medium to thick cumulus clouds [Deirmendjian, 1969] composed of non-absorbing (in the visible) water droplets.

The optical thickness of any scattering medium is seldom a simple function of the aggregate mass but depends rather strongly on the optical constants, shape, size range, size distribution, and number density of the particles. Smoke particles, of course, are not transparent and are much finer than either cloud drops or ordinary atmospheric aerosol particles, hence their extinction efficiency per unit mass should be considerably higher than that of cloud drops.

The NAS/NRC [1985] report cites an average extinction optical thickness of 4.0 for smoke distributed over the hemisphere, with a range of $0.44 < \tau_D < 14.3$. Surprisingly, this range is somewhat lower than that of our above estimate on the basis of Krakatoa's ejecta, and its direct connection to a nuclear winter is far from proven. This brings us to the famous Tambora (1815) eruption and its reputed climatic effects.

In a short, characteristically clear article, the well known climatologist H. Landsberg [1974] discussed the possible climatic effects of volcanic dust. Among other things, he reminds us that, as earlier pointed out by this author [Deirmendjian, 1973a], the volcanic "dust" from major eruptions cannot substantially deplete the total incoming global (direct plus diffuse) solar radiation, since the loss in the direct flux would be mostly compensated by the diffuse flux, due mainly to the near-sun forward scattering by aerosol particles. Observations under volcanic dust conditions would probably verify this. Thus, any attribution of the cold 1816 New England summer to Tambora's effect should be closely scrutinized, quite apart from the rather weak statistical evidence for any such effect found by Landsberg [1974].

At the conclusion of the chapter on "Fires," the authors of the NAS/NRC report list a number of uncertainties which must be considered in any assessment of smoke-related effects from nuclear operations. Prominent among these are uncertainties in the optical properties (especially the complex index of refraction) of urban and forest fire smoke particles which may range in value within factors from two to three. Such uncertainties, of course, are neither trivial nor negligible in estimating the absorption and scattering effects of particles that are finite yet small compared to the wavelength of the incident radiation [see Deirmendjian, 1969, and other sources].

Whereas a knowledge of the microphysical properties of smoke and other absorbing particles is crucial, the multiple scattering and hence radiative transfer effects, as mentioned, e.g., in the report [NAS/NRC, 1985, p. 132], are relatively unimportant, except, perhaps, in a gross way, in an initial study, particularly if interactions between the

radiation and dynamic fields are to be omitted. However, as the excellent survey paper by Prospero and his seven co-authors [1983] clearly demonstrates, even in the unperturbed atmosphere, such properties as the constitution, particle size, shape, size spectrum, concentration, etc. of *any* atmospheric aerosol ensemble, and hence its optical properties, are functions of source, geographical and vertical position, and residence time (described as "short" even for natural aerosols). Thus it would be rather difficult to predict the optical properties of a turbid atmosphere that has been strongly perturbed, both spatially and temporally, as would be the case following a general nuclear conflict.

Another major review of NW topics, which seems to have been undertaken simultaneously with the NAS/NRC one, is contained in the report [Hare, 1985] prepared by a special committee of the Royal Society of Canada composed mainly of Canadian scientists. Judging from the calibre of the committee members and that of its consulting scientists, the main body of the report [Hare, 1985, English language version, pp. 15-67] undoubtedly represents a serious effort resulting in a rather credible document. A "Supplement," covering a hefty 250 pages, contains some 16 "consultants' reports and technical papers" which deserve a more thorough review than is possible here. Most of the report's conclusions generally parallel those of its U.S. counterpart [NAS/NRC, 1985] but differ from it in tone and emphasis.

Thus in their principal conclusion [Hare, 1985, p. 52], despite the admitted considerable uncertainties involved in current modeling efforts, the Canadian reviewers make no bones about the seriousness of the NW problem and state that "a nuclear winter in the wake of a major nuclear exchange appears to be a formidable threat" and pronounce as "credible" the model predictions of severe drops in mid-continental temperatures. These assertions are made with somewhat more confidence than in the NAS/NRC [1985] report on the matter, and are in better agreement with the original TTAPS [1983] conclusions than is indicated by their early critics [see below and Sagan, 1985].

Moreover, the report [Hare, 1985, p. 56] offers quite definite recommendations to the Canadian government (as originally requested by their Ministry of the Environment) which include not only considerable additional NW modeling and other research, but also recommendations in the strategic and policy areas, such as were not contemplated in the NAS/NRC report.

Finally we wish to reemphasize the importance of the already-mentioned comprehensive overview by Prospero et al. [1983] on the atmospheric aerosol system and the very considerable advances in our knowledge achieved during the last 50 years or so. Although not directly related to the NW problem, the material presented in the above work is nevertheless highly relevant to it. In particular, these authors clearly demonstrate the extreme variability, complex chemistry, microphysics, and evolution of atmospheric particulates and, hence, the serious difficulties involved in attempts to predict the behavior of massive perturbations, such as the smoke from extensive fires, introduced by nuclear wars into the existing aerosol system.

2. **Other Work**: A major effort in NW studies is that undertaken by workers at the Lawrence Livermore National Laboratory (LLNL), according to an article probably authored primarily by M. C. MacCracken [1985] appearing in their house organ, *Energy and Technology Review (E&TR)*. The clearly presented and well illustrated article is essentially a description of what appear to be ongoing comprehensive studies on various NW-related subjects. Most of these studies were either unfinished or unavailable in published form at the time of writing. There are hardly any definite conclusions presented in the article worth mentioning here. We note that in a discussion of smoke effects, it is implied that the absorption by smoke particles is complete, whereas it is known from the theory of spherical particle scattering [Deirmendjian, 1969] that this is impossible (except under very special conditions) and that some finite portion of the incident energy is almost always scattered out by the particles.

Other published studies worth mentioning are as follows: A short paper by Malone, et al. [1985] finds that the estimated residence time of post-nuclear war smoke is greatly increased if solar heating and precipitation scavenging are taken into account (that is, under their preferred assumptions, some of which are of doubtful validity). A longer, just-published study [Malone, et al. 1986] by essentially the same team of workers from the Los Alamos National Laboratory and the NASA Ames Research Center, presents another simulation of NW conditions using the *three-dimensional* National Center for Atmospheric Research (NCAR) so-called CCM model. Although the "non-linearities...and closed feedback loops" involved in any realistic modeling of atmospheric processes are mentioned by the authors, the text and conclusions of their paper nowhere reveal how these non-linearities and interactions have been--if at all--specifically incorporated in their calculations.

Similarly, though not explicitly stated in their abstract and concluding remarks, the general impression given by this paper [Malone, et al., 1986] is that, considering the various stated (and some unstated) uncertainties in various important parameters, the reliability of their stated results cannot be very high.

An interesting paper by Ramaswamy and Kiehl [1985] investigates the sensitivity of radiative heating effects to variations in the microphysical properties, vertical distribution, total columnar amount, etc. of the large masses of smoke and dust involved in the NW hypothesis. The deduced surface cooling varies between 3 K and 32 K depending on the assumed range of the variables. Again the mechanism by which the smoke particles presumably heat the surrounding air is not discussed or specified [see, e.g., Fiocco, et al., 1975, 1976]. One may assume that the effects described (not always clearly) by the authors operate while other atmospheric processes--such as kinetic dynamics and precipitation--remain unchanged, i.e., various interactions have not been considered.

In a short but significant note Golding, et al. [1986], investigate the evolution of a single dense smoke plume by means of a mesoscale numerical model with a 15 km resolution (compared to a resolution of

several hundred km in the case of general circulation models). Their results indicate that, e.g., the vertical motions generated by the heating* due to sunlit smoke may produce condensation and even precipitation otherwise unpredicted by the large-scale data grid involved in global climatic models.

A brief and incomplete review such as this cannot possibly cover all the pertinent literature on the subject. We were unable to properly examine, e.g., the paper by Cess, et al. [1985] which seems to entail a serious effort to investigate the effect of some climate feedback mechanisms on the NW situation.

There are some other NW-related shorter pieces of work, however, that may also be mentioned. An example is that by Small and Bush [1985] on smoke from non-urban sources in a nuclear war. Their estimate of the equivalent optical thickness is 0.18 or a small fraction of that for urban fire smoke. Another example is a paper by Broyles [1985] on the general subject of smoke generation in a nuclear war with conclusions that are essentially in agreement with those of TTAPS [1983].

3. **Letters and other published comments**: Opinions pointing out weaknesses in the TTAPS and similar studies were put forth early in the game, e.g., by S. F. Singer [1984a], a well known atmospheric physicist. His remarks were later amplified in letters to *Nature* [Singer, 1984b] and *Science* [Singer, 1985] each followed by rejoinders [Thompson, et al., 1984a; Turco, et al., 1985] by the authors of the original papers criticized by Singer. The letter in *Nature* refers to an article by Covey, et al. [1984] on the use of a three-dimensional climate model to simulate the NW, the results of which essentially seem to agree with those of the TTAPS (one-dimensional model) as far as sub-freezing surface temperatures are concerned. Here most of Singer's criticisms

*Reviewer's note: In a number of conversations, Deirmendjian expressed a concern that many of the NW models treat the smoke layer as a black body that absorbs solar radiation and then heats the air around it by reradiation. This has profound effects on the dynamics of the atmosphere and the residence time of the smoke. In reality, most of the particles are small relative to the wavelength of the incoming radiation, so the black body law does not hold.

seem cogent, as the authors themselves admit [Thompson, et al., 1984a]. The initial use of the NCAR model does have weaknesses as pointed out by F. W. Murray [1985]. However, the same model, with some improvements, has been used at Los Alamos, and the results, as mentioned above, have just been published [Malone, et al., 1986].

An attempt to refute some of Singer's [1984a, 1984b, 1985] criticisms appears in a letter to *Science* by the TTAPS group [Turco, et al., 1985]. Their reply, rather than concentrating on a substantive refutation of the points raised, seems to attack the respondent's motives or to cite unpublished work "in preparation" in their arguments. In general, it would seem that some legitimate *scientific* questions raised by Singer and others with regard to NW have not been addressed by the TTAPS authors in a scientific context so far.

In a commentary appearing in *Nature*, Sagan [1985] offers a considered refutation--entirely in qualitative terms--to the TTAPS critics' arguments, citing the NAS/NRC [1985] and the Canadian committee's [Hare, 1985] results in support of the validity of the TTAPS main conclusions. He takes Edward Teller particularly to task for his 1984 critical comments before a Congressional subcommittee and elsewhere on the supposed NW threat and concludes with a discussion of related policy matters and the need to limit nuclear armaments.

Another exchange took place between the Soviet scientist Aleksandrov on the one hand and Thompson and Turco on the other [see Aleksandrov, 1984; Thompson, 1984b; Turco, 1984] about the validity of the climate model used by Aleksandrov to simulate NW climatic effects. Soviet work, by the way, especially in problems involving light scattering and the atmospheric radiation field, both of which are directly related to NW problems, should not be underestimated. Also, in the area of general circulation research, their models may be simpler than ours, but their results are probably no less valid, as pointed out by Murray [1984]. Incidentally, according to press reports, Aleksandrov disappeared without a trace or explanation, some time in March 1985 while attending an international workshop in Toledo, Spain, and is still unaccounted for as of this writing [Rich, 1986].

The proceedings of that first joint USA/USSR conference on the long-term consequences of nuclear war, held in Washington, D.C., October 1983, were edited and published by Ehrlich, et al. [1984]. Among early reviews, that by Rathjens and Siegel [1985] is somewhat critical of the book's catastrophic forecasts, with some justification.

Some earlier relevant comments, made prior to the NW hypothesis by notable atmospheric scientists, may here be recalled. The well known atmospheric scientist, C.-G. Rossby, in an otherwise unpublished public lecture at UCLA in 1952 or 1953, cautioned about predicting the effects of, e.g., increased CO_2 without considering the atmosphere's ability to compensate for disturbances. B. J. Mason [1977], the director of the British Meteorological Office, (as quoted by Deirmendjian [1980]) put it more tersely as follows: "...the atmosphere is a robust system with built-in capacity to counteract any perturbation....Sensational warnings of imminent catastrophe unsupported by firm facts or figures not only are irresponsible but are likely to prove counterproductive."

4. **Further comments**: The problems involved in the support and administration of NW-related analysis and "research" are reminiscent of a similar problem which, though smaller in import, was given much prominence a few years ago, namely, the possible consequences for the ozonosphere of the operation of a fleet of commercial supersonic transports (SSTs). The response was the so-called Climatic Impact Assessment Program (CIAP), which allocated to its administrator a certain sum and a certain amount of time, say, two years to come up with answers. As a result of the CIAP operation, numbers of atmospheric and other scientists (in great need of financial support at that time) submitted proposals to perform any work even remotely related to their own fields of competence, in order to procure some support. Results: A number of meetings, technical reports, etc., which culminated in one *Executive Report* [Grobecker, et al., 1974], and a collection of technical reports representing short term or "crash" work, which few knowledgeable workers seem to have cited or otherwise used since. In other words, no credible results seem to have come out of the CIAP

exercise despite the cost and the number of people involved. It would therefore seem that the "orchestration" and direction of the efforts of a large number of scientific workers, of various backgrounds and affiliations, by a science administrator to solve certain semi-scientific problems may not be the best procedure. (Although such an arrangement was considered earlier, current support for NW-related work does not seem to follow the CIAP example but is rather in the form of grants through individual organizations.)

As far as RAND's possible interest in this area is concerned, it may be recalled that about a decade ago, we had an in-house *general circulation model* (GCM) and computer program [Gates, et al., 1971] debugged, tuned, and operational, which, though not representative of the most advanced state-of-the-art, could probably still be used today in the modeling of NW-type experiments. (As a matter of fact, Aleksandrov and Stenchikov [1984] used the same program, acquired through the RAND documentation, but with much coarser resolution, for the Soviet NW studies.)

Interestingly, one of the simpler experiments, called "black cloud" [Kahle and Deirmendjian, 1973] that was run on RAND's in-house GCM, though inconclusive, involved NW-type boundary conditions in that the incoming solar radiation was assumed to be reduced globally and uniformly by 6.5 percent. A model called "dirty cloud" incorporating higher optical thicknesses and hence higher reductions in global radiation into the experiment was also proposed [Deirmendjian, 1973b]. The latter in turn was based on the results of yet another RAND study [Batten, 1966] on the weather and climate effects of nuclear war, which included an estimate of the mass of nuclear debris lofted into the stratosphere. The equivalent optical thickness derived from that estimate [Deirmendjian, 1973b], i.e., τ_D (λ 0.45) = 1.26, turned out to be just about twice the one deduced earlier for the Krakatoa dust [Deirmendjian, 1973a]. By the way, this value happens to be about three times the lower limit, 0.44, of the range in global optical thickness corresponding to nuclear debris mentioned in the NAS/NRC report [1985]. Considering extinction of the direct sunlight only, our "dirty cloud" model amounts to a reduction by 72 percent in the solar energy reaching

the ground. This is a large percentage, but still less than the catastrophic reduction predicted by TTAPS.

Undoubtedly our above-mentioned "dirty cloud" model, when suitably modified, could still be used in conjunction with RAND's own GCM as a first serious experiment to simulate NW conditions. The results would serve to check the validity of various realistic and significant mechanisms introduced into the model in order to understand the atmospheric response to such a monstrous event as all-out nuclear war. However, such a comprehensive study would be difficult to undertake without adequate scientific manpower.

As expressed in an earlier note [Deirmendjian, 1974], the meaningful introduction of particulate turbidity into a GCM that would include, e.g., interaction between the radiation and the dynamic fields, is not a simple matter. Introduction of higher-order effects such as the multiple scattering of sunlight by cloud and aerosol particles further complicates the problem but perhaps does not affect results much. The above-mentioned "black cloud" type of model is equivalent to a simple absorbing filter placed on "top" of the atmosphere.

The limited ability of current atmospheric science to predict weather within the essentially stable ocean-atmosphere system with some degree of reliability beyond 12 hours is well known [see, e.g., Ramage, 1976], despite some 40 years of research and its results as published in the scholarly journals. The operation of expensive and sophisticated satellite and other weather-observing systems does not seem to have substantially improved this situation [Kerr, 1985]. As a matter of fact, our limited ability to predict the results of mildly disturbed conditions--let alone those of a major NW-type anomaly, was amply demonstrated by the meteorological and climatological surprises accompanying the 1982/1983 "El Niño" anomaly [Kerr, 1986].

The recent widespread interest in the NW problem and the proliferation of related literature may be mostly attributed to the wide audience and compelling arguments of the TTAPS [1983] paper (rather than to Crutzen and Birks' [1982] earlier and original paper as might have been expected). The ever increasing numerical climate-modeling efforts and experiments are understandable since no real experiments with the

atmosphere itself nor a scaled-down physical model of it are possible. This is one reason why observation of, and data from, other planetary atmospheres, particularly from those of Mars and Venus, are so important as indications of the behavior of atmospheres vastly different from our own. (See, e.g., the eminently successful balloon experiments launched into the Venusian atmosphere by an international team of scientists, as described by Sagdeev et al. [1986a,b], Blamont et al. [1986], Kremnev, et al [1986], Linkin, et al [1986a,b], and Preston et al. [1986].)

No review of the literature on the biological effects (see, e.g., the article by Ehrlich and 19 co-authors [1983] or on the policy implications of the NW hypothesis will be attempted here. However, some comments on policy questions are in order since, due to the nature of our subject, such questions are apt to creep into--as indeed they seem to have--some of the cited papers which are otherwise concerned exclusively with the geophysical aspects of the problem.

When discussing policy questions related to the NW "scenario" against a background of relevant physico-chemical and geophysical results, one may be faced with some dilemmas: e.g., it has been said that even though the basic physical scientific results are of unknown reliability or verifiability (as in this case) policy decisions may have to be made. One way then is to ask how to decide on the degree of reliability (in the physical results) needed for valid policy judgments to be made; and who should make the decision--the authors of the physical results or the policy analysts who may not even understand them but make use of them.

One may also ask whether scientific/technical material should be intertwined with policy discussions within one and the same paper (as in some of the NW literature reviewed), or should these two areas remain immiscible and be presented in separate papers. In the first alternative, is it possible, e.g., for the author of the physical-scientific part to remain unbiased and uninfluenced by his own policy preferences in the presentation of his material? A fair peer review of such a manuscript for journal publication may be difficult. These and other questions must occur to other authors concerned with and engaged in NW-type problems, and of course no ready answers can be offered here.

5. **Concluding remarks**: No true conclusions are possible on the basis of this short review. The overall impression that may be gleaned from the literature surveyed, particularly from the reports of the U.S. [NAS/NRC, 1985] and Canadian [Hare, 1985] ad hoc committees, is not very different from our own [Deirmendjian, 1984] and others' [Singer, 1984a, b, 1985] initial conjectures or educated guesses: there certainly will be some more or less substantial transient perturbation caused by a major nuclear exchange, but its nature, magnitude, and duration cannot be assessed accurately at present.

As for the possible catastrophic or eschatological consequences of NW conditions following all-out nuclear war, it seems that, at least for many non-human life forms, their extinction during geologic times was not rare [Raup, 1986] and may be triggered again in this case. It would also seem that doing a lot of serious research on the possible secondary consequences of nuclear war would be meaningless, at least from the point of view of advancements in the atmospheric sciences. On the other hand, the absence of any discussion at all of such consequences will leave the policy analyst or strategist with no alternative but to create his own scenarios or options. A moderate amount of well conceived and executed effort at a few competent research centers enabling an intercomparison of results should produce the best answers on the NW problem.

Addendum: For another survey of some of the literature cited here, and for a commentary thereon from a somewhat different point of view, see Hahm [1985].

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